Reviewer 2

General Comments

This work employs a 1D channel subglacial hydrology model to study the effects of dynamically determined effective pressure on ice sheet stability at the grounding line of a flowline ice sheet model. The model employed in this study is a reduced version of the model presented by Kingslake and Ng (2013) (omitting lake evolution and distributed drainage system). The experimental design is an extension of that of Brondex et al. (2017) to include a dynamically determined effective pressure. The authors perform a dimensional analysis on the subglacial hydrology equations to study relative contribution from the involved processes. In the steady state, the authors find that effective pressure reaches a local maximum in the vicinity of the grounding line. The authors state that dynamically determined effective pressure results in faster and farther grounding line retreat than a perscribed effective pressure profile.

Thank you very much for the constructive, thorough review. Responding to them has indeed involved a non-trivial amount of work, including redesigning the sensitivity analysis, and rerunning all simulations, but has significantly improved the paper, in our opinion.

This study suffers from three foundational shortcomings: a lack of novelty, an overly simplistic model/experimental design, and non-physical treatment of the static effective pressure in the second transient experiment which forms the foundation for inferring that subglacial hydrology enhances marine ice-sheet retreat. Shoring up these shortcomings will require a non-trivial amount of work but will be a worthwhile contribution for understanding the impact of subglacial hydrology on grounding line positioning.

We recognize some of these shortcomings, and will describe how each will be addressed below. In summary:

- Novelty: as pointed out in the opening paragraph of this review and by the other reviewers, this paper represents an extension of previous work. For example, we take a similar approach as Brondex et al. (2017), but use a more physics-based representation of subglacial hydrology. This representation allows us to examine the physical controls on effective pressure and subsequent grounding-line retreat. This has not been achieved in previous similar studies and we therefore disagree that the study lacks novelty. Most importantly, our work examines a novel set of physical couplings between the flow of ice and the flow of subglacial water, yielding new insights into the physics controlling the coupled system. It also provides a physical basis for a commonly used parameterization of water pressure near the grounding line. We will add a sentence mentioning the latter point to the abstract.
- Simplicity: the model and experiments were carefully designed to balance sophistication/realism (for example, by including longitudinal ice stresses, channel water flow physics, and channel evolution physics) and simplicity (for example, by assuming a single channel, a smooth, idealised bed topography, and a uniform, constant accumulation). This has allowed us to observe new behaviour, such as the distribution of effective pressure and, crucially, to understand the physics controlling that behaviour. If our model or experiments were less simplistic, for example, if they included realistic

topography, a multi-component drainage system, or variable climate forcing, it would be much harder to gain that understanding. We argue that there is space in the literature for idealised reduced models that help us uncover and understand new dynamics that may be at play in the real system, just as there is a place for work using models that attempt to describe a wide range of known processes. The latter cannot be built and improved upon without the former, in our opinion.

- Effective-pressure treatment: One of our transient experiments (T2) keeps the effective pressure constant in time. This appears to be a source of confusion and we aim to clarify our approach below and in the revised manuscript. The reviewer points out this is unphysical. We agree. We impose this unphysical behaviour because it emulates the approach of large-scale ice-sheet modelling. One of our goals with this paper is to highlight the unphysicality of this approach and demonstrate the impact it could have on model behaviour - specifically, reducing simulated grounding-line retreat. When simulating contemporary ice-sheet evolution, spatially varying basal properties are typically determined with an inverse procedure that finds basal properties that allow the simulated surface velocity to best approximate measured surface velocities. These basal properties are then kept constant in time. This is what keeping effective pressure constant in time in experiment T2 emulates. The fact that this approach leads to reduced grounding-line retreat potentially has implications for employing this approach in ice-sheet modelling.

Specific Comments

The key finding that effective pressure exhibits a local maximum close to the margin is not itself novel. Others have found an increase in effective pressure toward the ice stream terminus (around 10/13 models, de Fleurian et al., 2018) or grounding line (Dow et al., 2018). It does, however, give the reader more confidence in the model to see it replicate known behaviour.

We agree that one of our findings supports and elaborates on previous findings that effective pressure exhibits a local maximum close to the margin. We do not claim that we are the first to recognize this. We instead try and elaborate on how this arises. We will refer to these previous findings by others directly in our discussion, around line 416, and emphasize how our results are replicating known behaviour.

The inclusion subglacial hydrology to more realistically capturing margin retreat has been done before (e.g. Brondex et al., 2017). The authors state that studies which invert for sliding coefficient on the basis of measured surface velocities underestimate the potential for grounding line retreat and that the inclusion of dynamically determined effective pressure from a subglacial hydrology model reveals enhanced grounding line retreat versus static bed conditions. This conclusion may be a consequence of comparing against a static bed condition which imposes an effective pressure profile which does not change as the ice at a given location thins. This means that the static bed condition case is stabilized by an effective pressure condition from previously thicker ice. These experiments should be redone to reflect ice thinning, for example by imposing an effective pressure as a fraction of overburden as detailed in the response to Fig 7 below.

Yes, our work builds on that of Brondex et al. (2017) to include a subglacial hydrology component that allows for additional points of coupling. Our inversion for this static bed condition is motivated by existing models which invert for a temporally unvarying basal friction parameter. This is almost like imposing an effective pressure profile which does not change as the ice at a given location thins. You are right that in the static bed condition case, grounding-line migration is stabilized by conditions from previously thicker ice. As explained above, this is something we wanted to examine the implications of, given that it is a widely used approach.

Regarding the suggestion to redo the experiments to “reflect ice thinning”, our *coupled* transient experiment (T1) does account for ice thinning because the ice thickness affects the water flow and channel growth (as well as accounting for ice velocity affecting channel growth, and water pressure affecting ice flow and therefore ice thickness). Imposing an effective pressure as a fraction of overburden is a useful idea. Perhaps this can be considered a compromise between the more physics-based T1 and the unphysical (but representative of common ice-sheet modelling practice) T2. We have experimented with variations on this suggestion. For example, a reasonable modification to the suggestion is to scale the effective pressure/overburden ratio (N/P_i) horizontally so that the ratio shrinks or stretches with the model domain as the grounding line retreats or advances. Another approach is to assume full hydraulic connectivity to the ocean (Tsai et al., 2015). This is the approach we compare our model to analytically in the discussion. These, and others, are interesting potential parameterizations to explore and it would be interesting to examine if and under what circumstances they yield similar results to our coupled model. However, we argue that this is beyond the scope of the current paper. Our focus is using a simple comparison between a static-in-time, variable-in-space, bed assumption (that can be considered the first-order approach; a zeroth order approach would be to ignore spatial variability as well) to our physics-based model to understand some of the physics of the coupled system. Our focus is not to examine alternative, more complex effective-pressure parameterizations, though this could be a fascinating follow-on study.

Furthermore, it should be shown that these findings are robust to varied choices of parameters. For example, how does grounding line retreat differ for the static and dynamic cases for different accumulation rate? This study would benefit from a sensitivity analysis of the conclusions to scale decisions in the experimental design (flow law constant, accumulation rate, additional water source term, and input water flux boundary condition). Or better yet, an internally consistent calculation of some of those quantities.

We agree that ideally our transient experiments would be run repeatedly over a range of parameter values. Unfortunately, the computational resources needed for this make it highly challenging to perform a comprehensive parameter sweep. Therefore, we followed previous similar work (e.g., Brondex et al., 2017) in neglecting such a sensitivity experiment of the transient experiments. We do however include a redesigned sensitivity analysis of the steady state findings.

This paper has uncovered some new model behaviour and examined the physics of this in detail. A useful next step will be to see if the details of this behaviour depend on model parameters. We consider it reasonable to include this in a future publication.

Given this, we acknowledge that the title of the original submission could be interpreted as too absolute. We have changed it to “Two-way coupling between ice flow and channelized subglacial drainage enhances modeled marine ice-sheet retreat”. It now includes ‘modeled’ so that it is clear that this is a statement about how our model behaves rather than a more general statement.

It is a worthwhile exercise to examine the marginal contribution of isolated processes to overall system behaviour by turning off other interacting processes. In the earth system, however, neglecting fundamental processes and their interactions breaks the relation between model and true system. Here the authors employ a subglacial hydrology model forced with prescribed melt input. In the true earth system, this melt input comes from strain heating, basal frictional heating, and surface melt sources. This model is missing an important set of feedbacks between temperature dependent ice stiffness, basal melt production, hydrologically controlled basal friction, and ice-thermomechanics. Including thermomechanics in this model is feasible given its simplicity and would allow extension of model behaviour closer to the true system.

We appreciate the acknowledgement of how focusing on isolated processes can be a worthwhile exercise. Including thermomechanics is feasible, but we chose to exclude that from our scope in the interests of simplicity. It is true that our model is simpler than reality. However, choosing whether or not to include additional physics is a compromise between simplicity, which greatly aids interpretability, and realism, which can be useful but is not the only factor. For example, because we exclude melt-sliding-hydrology feedbacks, we are able to attribute the interesting model behaviour we observed to other processes. Without this exclusion, it would be more difficult to do this. One negative outcome of this exclusion is that we need to impose a poorly constrained melt water supply term. However, it is important to note that including ice-bed-frictional melting would entail a similar issue; we would need to prescribe the width of channel catchment in the across-flow direction. Finally, note that we are focused on a set of important couplings that have not been looked at together in this setting, whereas melt-sliding-hydrology couplings have been studied in similar models and settings previously (e.g., Robel et al., 2013).

The perturbation used in the transient experiments is not physical. The time scale for surface temperatures to propagate to the full thickness of the ice body is given by the thickness over the accumulation rate. Given your ice thickness of 1000 m and realistic spatio-temporal mean Holocene accumulation rates for West Antarctica of 0.27 m/a (Bodart et al., 2023) or up to 0.40 m/a modern accumulation rates closer to the coast of West Antarctica (Kaspari et al., 2004), it would take about 2.5 kyr for surface warming to propagate through the ice column – far longer than the 10 yr period examined in the transient experiments. A more realistic approach would be to impose a change in buttressing at the grounding line to simulate ice shelf break up, allowing self-consistent evolution of the ice stiffness with thermomechanics. This would also allow for comparison of the importance of subglacial hydrology to buttressing for Antarctic mass balance and grounding line stability. In the context of the processes which determine grounding line position, where does subglacial hydrology rank?

We agree that instantaneous temperature change is unrealistic. The motivation for this idealized approach came from Schoof (2007), which used this approach in an idealized model. In the revised manuscript, we instead perturb the ice-shelf buttressing, which is parameterized by a multiplier in the ice-stress boundary condition at the grounding line, consistent with the experiments done by Brondex et al. (2017). This adjustment to the experimental design does not affect our results qualitatively and we will edit the methods and the results to account for this new perturbation.

We think that a significantly more complex experimental design would be required to tackle the interesting question the reviewer poses “In the context of the processes which determine grounding line position, where does subglacial hydrology rank?” but this would definitely be an interesting question to ask in future work.

The summary accumulation rates discussed above also point to an issue with the parameter ranges in the experimental design. No justification is given for those values and in the case of accumulation, the upper bound for the sensitivity experiments is far too high (10 m/a) and the mean value used in the other experiments is too high (1 m/a). Because of the sampling methodology used in the sensitivity analysis, the realistic accumulation rates are undersampled. This is also the region (your Fig. 2) where your model displays the greatest sensitivity to accumulation rate – in terms of effective pressure, ice thickness, and grounding line position. The parameter ranges in this study need to be revisited, adequately justified, and more appropriately sampled (see note below for line 190).

We agree. We have changed our sensitivity tests to use realistic bounds, and to also examine interaction terms. We describe the bounds in detail after your note for line 190, and we have modified our discussion based on these new results. We’ve also modified our remaining experiments to use realistic accumulation rates.

The dimensional analysis in steady state of the model processes was a welcomed inclusion and served to highlight important model components. In all figures other than Fig 5, however, showing the non-dimensionalized versions of the variables made their interpretation cumbersome. These should be changed to the dimensional versions.

We are pleased that the reviewer found the dimensional analysis useful. We will change Figure 4 (Figure 2 in the revised manuscript) so that it plots dimensional variables. However, in Figures 3 and 4, we do not plan to change the nondimensional distance. It is useful to plot against the nondimensional distance because it allows for easier comparison between different simulations, which in general result in different domain lengths.

Itemized Review
Numbers refer to line numbers unless otherwise indicated
– 21: Seroussi et al. (2020) does not discuss subglacial hydrology, they only refer to de Fleurian et al. (2018), please remove.
We will remove this citation.

– 27: Also see Alley et al. (1994)
This is another example of hydrology affecting ice dynamics, and we will include that in our references.

– 33: Drew and Tarasov (2023) shows varied ice sheet behaviour by parameterization and inclusion of varied hydrologies.
We will include this citation along with Kazmierczak et al. (2022) after the statement about ice sheet dynamics and varied hydrologies

– 38: But there are plenty of examples of other subglacial hydrology models of varying degrees of complexity from intermediate (Bueler and van Pelt, 2015; Drew and Tarasov, 2023) to high (Werder et al., 2013; Sommers et al., 2018). I think this sentence could be removed.
Agreed, we will remove this sentence.

– 50: Consider discussing Dow et al. (2018) application of GLADS to marine terminating ice stream but for one-way coupled unsteady conditions. They also find increased effective pressure at grounding line.
This is a useful paper, and we will discuss it at line 366 along with other findings of increased effective pressure near the grounding line.

– 80: citation needed
We will add a citation for the model (Schoof, 2007).

– 102: add e.g. to citation 70
We will add “e.g.” to this citation

– 167: remove respectively
We will remove the word “respectively”.

– 170: a plot of the relative positioning of hydrology and ice dynamic variables and their grids would help here
We do not plan on adding another figure here showing the grids. We think that the written description of the grids is sufficient. We are happy to revisit this decision if the editor considers such a figure important.

– 190: How were these parameter ranges decided upon? Please provide justification/citation.
In line with the earlier comments, we have determined more realistic ranges and provided justifications. The sensitivity test choices are updated as follows in our methods:

We conduct a separate sensitivity analysis using each of our two sliding laws. We vary four parameters while using the regularized Coulomb law: the accumulation rate a, the meltwater supply M, the ice stiffness A, and the sliding law coefficient C_C. For experiments with the Budd law, we vary three parameters: a, M, and A. We use four different values for each parameter. We limited the sensitivity analysis to these parameters and four different values per parameter to reduce computational time and to aid in visualization of the results. We selected the parameters that alter alpha and gamma in different ways. For example, raising C_B and raising A both reduce alpha, so we only examine sensitivity to A for simplicity.

We will introduce these new tests in Section 2.5.1, and discuss these new results in an updated Section 3.1.

The sampled values were chosen as follows.

- Accumulation rate, a: we linearly vary between 0.1 and 0.5 m/y to encompass realistic accumulation rates suggested by the reviewer (Bodart et al., 2023; Kaspari et al., 2004).
- Additional meltwater supply, M: we logarithmically vary between 1e-5 and 1e-3 m^2s^{-1}. This is obtained using realistic water fluxes at the grounding line, Q_{Out}, ranging from 1 m^3s^{-1} to 100 m^3s^{-1} (Dow et al., 2022; Hager et al., 2022) and then dividing by an ice sheet length scale of 100 km.
- Ice stiffness, A: we logarithmically vary between 3.9e-26 and 4.2e-25 to encompass the range of stiffnesses used by Schoof (2007).
- Coulomb sliding law coefficient: we linearly vary between 0.1 and 0.5. This is to encompass values of C used in studies by Helanow et al. (2021) and Pimentel et al. (2010).

We then update our S1 experiments to be run with the median of all the sampled parameters. For our transient experiments, we use a combination of parameters that allows for the initial ice-sheet geometry to extend past the overdeepening in the bed geometry.

– 190: how was the base vector selected? Did you try multiple base vectors? One at a time sensitivity analyses can be very sensitive to fixed values and is problematic for non-linear coupled models (Saltelli et al., 2019).

We no longer conduct our sensitivity analysis in a one-at-a-time manner following this and the below suggestion. We selected the base vector for our high resolution example to correspond to the median value of each parameter.

– 190: 100 values is quite a lot for a one-at-a-time sensitivity analysis. If able to run the experiment for this density then the sensitivity analysis would benefit from including interaction terms. See for example (Saltelli, 2002). Also, a uniformly spaced sampling is not valid for sampling across several orders of magnitude (Qin varies across 4), the lowest value sampled is actually 0.1 not 0.001 as stated in the text. The plots in fig. 2 show the greatest change at the lower values of each parameter range. Likely the zone of greatest sensitivity has not been sampled and is not displayed (e.g. As is stated to be varied between 0.01 and 100 but the lowest value actually sampled would be 1. Log uniform is more appropriate.

We agree. We have updated our sensitivity test and sampling to accommodate these suggestions, as described above. Figure 3 and 4 (now 2 and 3) have been completely redesigned as a result.

– 190: a table of the variables probed and their ranges would help here, perhaps an additional column in Table 2.
We have detailed the variables probed and their ranges as an additional column in Table 2.

– 198: is this hydrology resolution in agreement with table 1?
This is a typo and has been updated to the table 1 values.

– table 3: it would be helpful to see the equations in the centre column reduced to the constants in table 2 and the imposed scales in the first 3 rows. This would help to follow the scale choices through to the impact on process.
Thanks for the suggestion. We have reduced the equations and updated them in the table.

– 235: It looks as though these results are past the CFL limit for an explicit time step length. Taking your hydrology grid resolution as 100 m and the water velocity as Q0/S0 ≈ 0.5m/s, your time step should be less than 200 s (≈ 6e − 6 yr), but in your transient experiments you are using time steps of 5 yr and 0.05 yr. You are, however, solving via implicit backward Euler which is stable at larger Courant numbers. How sensitive are these results to your time step length? Do they converge under decreasing time steps? At such large Courant numbers time convergence needs to be checked when dealing with highly-non linear systems.
We agree that capturing fully channelized drainage dynamics would require smaller timesteps that adhere to the CFL limit. However, we emphasize that based on our scaling analysis, we assume that the subglacial channel is in a pseudo-steady state, meaning that the ice thickness and velocity evolves slowly enough that the channel size is always effectively in a steady state. This allows us to solve a steady-state version of the hydrology model in each time step, avoiding the requirement to meet the CFL condition for the water flow. This is now explained at the end of section 2.4, where we say: “We exploit this to simplify the numerical solution of the hydrology equation, by setting the time derivative in Eq. (12) to zero”. We also include a convergence test (Appendix D), and we see that the first 20 years of simulation converge to the same grounding line position as the time steps shrink.

– 244: Holding N constant for the T2 experiments neglects any change in coupling due to ice thinning and removes an important positive feedback. Perhaps a more reasonable approach would be to hold N as a fraction of overburden determined by the steady state conditions prior to perturbation? Then you could include that critical positive feedback of ice thinning in your T2 experiments.
As discussed above, the purpose of experiment T2 is not to include critical feedbacks (we agree, it does not). Rather, its purpose is to examine the impact of not including some critical feedbacks. This is motivated by the common practice of inverting for basal properties beneath contemporary ice sheets and keeping those properties constant in time.

– Fig. 2: please change horizontal axes to log scale for those parameters which vary over several orders of magnitude. Also changing the distance to actual physical distance instead of normalized distance would help the reader to recall the scales involved.
This figure has been changed to reflect our multidimensional sensitivity test that also looks at interacting terms, and our new Figure 2 includes logarithmic scales and gives dimensional grounding-line positions.

– 317: It is not appropriate to call the ice and hydrology fully coupled here, as no melt from basal, englacial, or surficial surfaces is included in a self consistent way here (e.g. strain heating or surface melt due to lowering below the equilibrium line altitude). Also S2 is not coupled to an ice model but forced with a profile (Eq. 19) and constant velocity.
We agree; our model neglects that important set of couplings. We will avoid using the term "fully-coupled" when referring to our coupled model, and we will refrain from referring to S2 as coupled. We will instead refer to S2 as a hydrology model with an enforced ice profile and velocity.

– 325: It does not follow that the region of higher basal shear stress is caused by the higher effective pressure. If no hydrology were included (e.g. Weerman sliding) you would still get this increase in basal shear stress here because of the higher driving stresses.
It is true that using a Weertman sliding law would result in high basal shear stress in this location *if* the driving stress were high in this location. However, without a region of high basal shear stress in this location the driving stress would not be higher in this location. In other words, we argue that it is the high basal shear that causes the ice thickness to evolve such that the driving stress becomes high in this location, rather than the other way round. We have left this sentence unchanged.

– Fig 7: This grounding line stability in the T2 versus T1 model setups may be a consequence of assuming an effective pressure which is independent of ice sheet thickness. For example if one assumes SIA – $\tau b = \tau d$ – and compares the effect on ice velocity from effective pressure proportional to ice sheet thickness ($N = aPice$) or constant then using your Budd sliding law you get $u = \rho igh\ \partial h-b\ \partial x\ /CbNconst$ versus $u = \partial h-b\ \partial x\ /Cba$. In the constant effective pressure case the velocity reduction due to ice thinning will be much greater than in the effective pressure proportional to overburden case. Furthermore, as the margin retreats into steadily increasing effective pressure you are effectively reducing the sliding coefficient. In this plot your velocity increase from the ice stiffness perturbation is at least half (plot seems to be cut off) in your T2 scenario versus T1. This disparity should likely be much less and is possibly the reason for the grounding line bifurcation.
Yes, we agree with this interpretation. The stability is a result of the assumption that this static effective pressure does not change with the ice thickness and the grounding-line position. As described above, this suggested parameterization of effective pressure may prove useful for describing temporal changes as an alternative to the simplest approach used by many models currently. Future work could investigate this.

– 359: The driving stress does not decrease because of the lower longitudinal stress increase, driving stress is determined by surface slope. In your Budd sliding equation the basal shear stress increases to balance the driving stress. This means velocity increase and ice thinning. Please restate.
Yes, we can see how the way we explained this was confusing. We will rephrase four sentences starting at line 358 as follows: “The removal of buttressing results in a reduction in longitudinal stress and an acceleration in ice flow near the grounding line. In T1, effective pressure is near zero immediately upstream of the grounding line so this acceleration does not lead to sufficiently increased basal shear stress to counteract the reduction in longitudinal stress. The acceleration leads to thinning and grounding-line retreat. In contrast, in Experiment T2, when N does not change with time, grounding-line retreat corresponds to a higher N near the boundary (and therefore higher basal shear stress) which grows to balance the reduction in longitudinal stress stress, preventing further acceleration and retreat.”

– 361: Again, this higher N near the boundary is non-physical given thinner ice from retreat.
We agree. See our discussion above about the purpose of emulating the non-physical approach taken by many ice-sheet modelling studies, to examine the consequences of this.

– paragraph starting at 390: I am confused by the logic here. Applying Eq 21 “which holds for most of the ice sheet far from the grounding line” to conditions right at the grounding line and inferring “that the advection term must grow large near the grounding line.” Perhaps the reasoning here can be better expounded in another way or reworded?
We can see how this could be confusing. The original text tried to point out why Eq. 21 cannot apply at the grounding line (because it would imply an infinite S). We will reword the argument so that it is made initially only in reference to Eq. 12 and the connection to Eq. 21 will only be made parenthetically and to support the argument that S grows large until the advection term becomes large enough to play a role. The sentence that started on line 392 and the next sentence will be replaced with the following:
“The channel in this region therefore grows until the advection term $\beta u^* \partial u/\partial x$ grows large enough to counteract the melt opening. We note how the fact that Eq. (24) only applies in regions far from the grounding line supports this interpretation; Eq. (24) shows that, for a channel with Q != 0, N = 0 requires S to be infinite. This is not physical, but it is conceptually consistent with S growing large near the grounding line until the advection term, which is neglected in the reduced model, starts to play a role.”

– 397: This reasoning feels circular in context of previous paragraph. Also, generally one would expect that as channel cross section gets bigger, flux gets bigger, and effective pressure gets bigger. Effective pressure drops because the ice is thinning toward the margin.
In a sense, the reasoning *is* circular because N and S are self-consistently coupled and in some instances, particularly when we can assume a quasi-steady state, it depends on your perspective which you consider is *causing* the other. To help get this idea across in the paper we will change the first sentence of this paragraph to say “Another way of looking at this is that channel cross-sectional area S growing large at the grounding line facilitates N dropping to zero.”

For clarification, we note that in this case the flux is fixed by water-mass conservation, so it is not the case that we expect a higher flux for a larger channel.

– 399: do you mean $\psi \approx -\partial N\ \partial x$?
Yes, this was a typo. It will be corrected.

– 414: which results?
This will be clarified to be “our steady-state experiment results”

– 423: “also be explained by coupling” – what aspect of the coupling?
We will clarify this as follows: “explained by coupling between between ice geometry, basal hydraulic gradient, subglacial-channel size, and effective pressure”.

– 430: Again, this is may be simply a consequence of the choice of effective pressure profile.
Yes, it is due to the choice of holding the effective pressure profile static.

– 435: “holding it static is similar...” – but it is not, when the ice retreats into the region of non-physically higher effective pressure you are effectively reducing the sliding coefficient, not just holding it constant.
It is similar because in the case when C_W is held constant in time, its spatial variability also effectively includes spatial variability in N, even if the sliding law used in the inversion does not include the effective pressure.

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